Responses to reviewers

Investigations of boundary layer structure, cloud characteristics and vertical mixing of aerosols at Barbados with large eddy simulations

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Dear editor and reviewers,

we thank you very much for your helpful comments on our discussion paper. In the remainder of this document, we describe the changes that we have made to the paper for final submission. The exact changes made in the manuscript compared to the last version are provided in a separate file using the `latexdiff` package.

Response to anonymous referee #1

Comment #1: The model numerics and physics are not new or unusual, so the equations should be presented as an Appendix or Supplement, if presented at all. This applies to section 2.1 and much of section 2.2.

> **Answer:** The Sections 2.1 and 2.2 show most important model features that are important for simulations. We already tried to keep them short, but the main ideas should still appear in the main text. We agree that some passages could be shorter, thus we moved Sec. 2.1 to the appendix and shortened Sec 2.2 a bit. This shifts the emphasis to the CP method, which is a new aspect in the context of the simulation setup.

Comment #2: I am concerned about whether the simulations are sufficiently resolved to produce converged solutions. A 200x200x50 m grid is much coarser than considered adequate for simulations of the convective boundary layer, which these days are usually done at 50x50x20 m or finer. This is particularly a concern for behavior in the stably stratified parts of the column, that is, near cloud base and the inversion. The aerosol layers of interest are also in stable stratification. The authors show that coarser simulations are not acceptable, but they do not show that the presented resolution is adequate.

At least there must be some explanation why the authors think this resolution is sufficient. As one example, at the end of section 4.3, stronger fluctuations in the measurements than in the model could indicate poor resolution.

Answer: First of all, finer scales on our 100x100 km^2 domain are hard to apply at the moment due to computational limitations. We consider the performed simulations as a first step toward island effects investigations with LES. Most other studies are dealing with this topic using coarser resolutions. Moreover, one has to be careful with the concept of numerical convergence in the context of LES modeling. In fact, as the grid is refined, there are more resolved scales, therefore being difficult to talk about solution convergence. There are several studies in the literature where coarser resolutions than ours are employed and also without proper inflow boundary conditions (cf. 1 Introduction). In these terms, we can make the case of the improve of the current work. Still, you have a fair point in stating that there can be some aspects that can suffer from the grid resolution employed (the stably stratified flow at the inversion is probably the one to mention here). Looking at the energy spectra in Fig. 6, it is shown that some portion of inertial range scales is resolved in the convective boundary layer, therefore being the modeling in that context correct. The authors already state in the Conclusions section that finer grid spacings are desired to reveal additional effects. Regarding manuscript changes, we added and edited some sentences to reinforce these points.

Comment #3: In figure 12, it seems that the cloud base is higher than the BL height for NOCP and DX400 around 0900. If this is true, it indicates a problem with the diagnoses.

Answer: We rather think that this is a problem with the laminar upwind state and/or coarser resolutions in these cases. The diagnosis is straightforward. The Bulk–Richardson–number criterion is used to determine the boundary layer height. The cloud base height is defined as the height where $q_c > 0.01$ g kg^{-1}.

Comment #4: We need a better explanation of why the day used for wind measurements was not simulated.

Answer: The explanation was definitely missing in the paper. The reason for different days is that no nighttime radiosonde was available on 20 June 2013. Available dropsonde data do not cover all altitudes, especially near-surface levels.

Comment #5: I didnt learn anything new from section 4.2 and figure 18. Perhaps they should be removed. In any case, points with low significance should be removed from the figure.

Answer: We re-structured Section 4 and focus on the most significant findings. Figs. 18+19 have been removed.

Comment #6: Figure 22 and its discussion: Im not convinced this is good agreement. Please clarify the figure or the discussion.

Answer: The mean vertical velocity profile is in good agreement from the authors' point of view. We still changed the sentence a bit to soften the language. The reasons for variance discrepancy can be 1) a result of lower effective model resolution (cf. spectra at high wavenumbers in Fig. 6), and 2) due to differences in the forcing. Stronger surface heat fluxes should increase variance of vertical velocity and likely produce a better agreement with the observations.

Comment #7: With respect to the aerosol, mixing and movement are not the same, and must be more clearly distinguished throughout the paper. In particular, there is no such thing as positive or negative mixing, although movement can be upward or downward. Similarly, waves do not produce mixing unless they break, but they can produce movement.

Answer: We changed the parts to be more precise in distinguishing between movement and mixing.

Comment #8: p.22649, line 8: Is this the initial wind, the geostrophic wind, or both?

Answer: It appears to be better to just call it "initial wind", so we changed this in the manuscript.

Comment #9: p.22653, line 2: What does ”three square cells” mean?

Answer: It means that the cells where the cell perturbations are applied have a quadratic base area.

Comment #10: p.22655, line 8: Do you mean latent heat flux?

Answer: Yes, we corrected this typo.

Comment #11: Figure 13 shows too many lines and cannot be understood. Some of the times can be removed without loss of information.

Answer: Due to the time-dependent forcing we wanted to show the diurnal variation of the vertical profiles of cloud and turbulence parameters. These 4x4 panels enable to figure out differences between the considered cases with the addition of their temporal information. We would like to keep them as they are.

Comment #12: Figure 14: A log scale for the concentration might make the diffusion more visible, it is quite difficult to detect now.

Answer: We replaced this figure with a new one, where contours of the concentration are shown in a log scale.

Comment #13: Figure 15 has the same problem and solution as figure 13.

Answer: Cf. answer to Comment # 11.

Comment #14: p.22663, line 3: This is really a downward movement, not mixing. See general comment 7 above. The two should be more clearly distinguished here and throughout the paper, for example in line 25 on the same page.

Answer: We agree on this and corrected the respective sentences where vertical movement or transport takes places rather than mixing.

Response to anonymous referee #2

Comment #1: Equations/numerics. It appears that cell purtubation method is something interesting of this manuscript. If so, I would recommend authors to focus on this part in section 2, while put only a brief description of other parts of the model in the main text to keep the text flow. Details of equations/numerics can be put in the appendix.

Answer: This is in agreement with the first comment from Anonymous referee 1. We shortened this section to bring the new method into better focus.

Comment #2: There are quite a few papers on the Saharan dust layer transporting to Barbados, as well as their radiative effects on local temperature and surface energy budget. These papers should be summarized and linked to the numerical simulations here, so that readers can see the link of simulations with the real world.

Answer: We added relevant references to the text.

Comment #3: Section 3.3, dust layers arrive at Barbodos having mean base height of about 1.5-2 km asl. Need a reference for this. Studies in Puerto Rico Dust experiment actually showed that mean dust extinction peaks below 1 km asl (as supported by Figure 10 in the following paper. Authors should acknowledge this difference. Wang, J. et al., 2003, GOES 8 retrieval of dust aerosol optical thickness over the Atlantic Ocean during PRIDE, J. Geophys. Res., 108(D19), 8595, doi:10.1029/2002JD002494.

Answer: In your reference, there is a maximum in the extinction coefficient below 1 km, which rather means that there is a maximum mixture of marine aerosol and mineral dust. Our statement and the choice of height levels for the model tracers are based on Raman lidar measurements conducted during the campaign. Especially the particle depolarization ratio shows that the pure dust layer begins at around 1.5 km altitude. An analysis of dust layers during the campaign can be found at

S. Groß, V. Freudenthaler, K. Schepanski, C. Toledano, A. Schfler, A. Ansmann, and B. Weinzierl: Optical properties of long-range transported Saharan dust over Barbados as measured by dual-wavelength depolarization Raman lidar measurements Atmos. Chem. Phys., 15, 11067-11080, 2015. We overhauled the text a bit and also refer to our subsection in Section 4 (cf. the comment at the end of this document).

Comment #4: Figure 4. This is a very idealized case, which is perhaps ok for large-eddy simulations. however, authors should acknowledge that dust layer has an important effect on solar energy budget. Again, during the PRIDE, it was found that dust layers can affect both long wave and shortwave radiation up to 80 Wm-2 (Figure 4 in the following refer- ence). Hence, treating dust aerosols as a passive tracer without allowing any radiative feedback have caveats, and authors should acknowledge this in the manuscript. Wang, J. et al. (2004), GOES-8 Aerosol optical thickness assimilation in a mesoscale model: Online integration of aerosol radiative effects, J. Geophys. Res., 109, D23203, doi:10.1029/2004JD004827.

Answer: We totally agree and have stated this in the modified manuscript, including your reference. One has to keep in mind that the simulation setup is semi-idealized, which means that for some quantities (e.g., atmospheric state) we directly used the available data and for others, assumptions had to be made. The latter case is necessary because there is either no data available or to enable a better interpretation of the results (cf. the sensitivity test with halved sensible heat fluxes) or to save computational time (e.g., when using a radiative transfer model on-line).

Comment #5: Vertical distribution of cloud and dust layers. There is common to see boundary layer clouds over islands surrounded by oceans. This review wonders if authors can present a figure showing the vertical distribution of dust and clouds. Reliable simulation of the relative position between aerosols and clouds is important to understand aerosol radiative effect. For an example, in the maritime continents in Asia, Ge et al. found that smoke aerosols above clouds can lead to more warming, and how the vertical distri- bution of smoke is regulated both by boundary layer process and land/sea breeze,and radiative feedback, and therefore, have a clear diurnal cycle. Dust particles are similar as smoke particles that they absorb solar radiation, and so their vertical distribution in PBL and with respect to clouds are very important. These points can be acknowledged in the abstract as to why we need to simulate the cloud aerosol vertical distributor correctly in the model. Ge, C. et al. (2014), Mesoscale modeling of smoke transport over the Southeast Asian Maritime Continent: coupling of smoke direct radiative feedbacks below and above the low-level clouds, Atmos. Chem. Phys. ,14, 159-174.

Answer: The vertical distributions of liquid water content and relative tracer concentrations are shown in Fig. 13 and Fig. 15. We are aware that our analysis neglects radiative feedbacks. This would be a next step for model development. We added your statement as well as your literature example in the Conclusions section.

Additional remarks

First of all, we have added three additional co-authors due to their contributions to this paper. O. Knoth as the creator of the used model ASAM assisted in performing all the simulations and helped with technical problems. M. Haarig and A. Ansmann contribute with additional data from the BERTHA lidar measurement system during the SALTRACE–1 campaign. We therefore added a subsection within Section 4. The following figures have been modified: 5, 7, 14. Two figures have been removed: 18, 19. Two figures have been added: 21, 22.

Kind regards,
the authors